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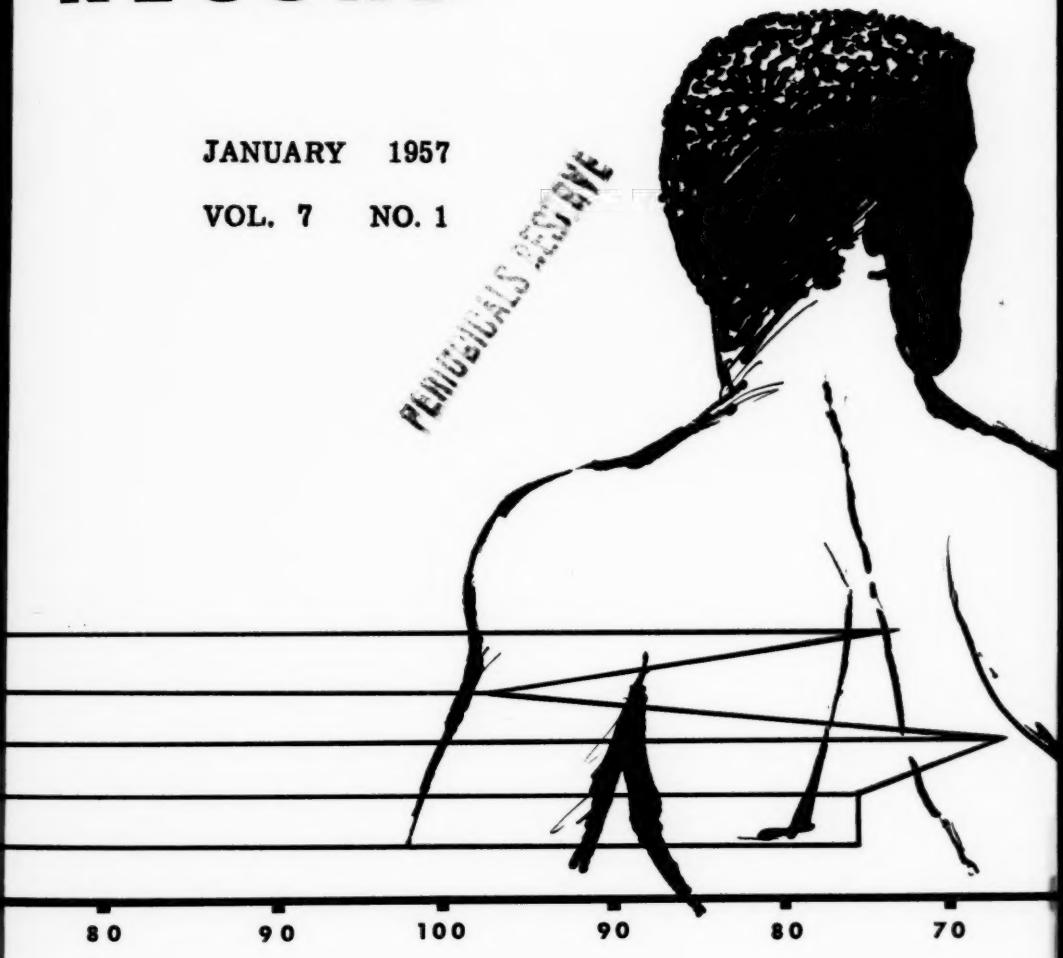

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THE PSYCHOLOGICAL RECORD

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CONTENTS

Birthday Greetings 1

A Review of the Conceptual Status of Anxiety: David Laskowitz 3

Vision with Spatial Inversion: A Follow-up Study: Fred W. Snyder and Charles W. Snyder 20

Perspectives in Psychology: David T. Herman 31

The Proper Study of Mankind is Rational Man: A Review of A Study of Thinking by J. S. Bruner, J. J. Goodnow, and G. Austin: Reviewed by Edith D. Neimark 35

Quarterly Commentary 40

BIRTHDAY GREETINGS

This issue of the revived Psychological Record is an anniversary publication. We have achieved our first birthday. To all who have participated in the realization of this event, to contributor and subscriber alike, we say: "Happy Birthday."

The coming year promises to be an exciting one. Edith Neimark has joined our staff as Book Review Editor. Dave Herman will write and edit a regular featurette, "Perspectives in Psychology." An appropriate subtitle for this latter department might be "Musings of an Interbehavioral Psychologist." Also, to each issue the editor will contribute a "Quarterly Commentary."

In the present issue of the Psychological Record we feature an article by David Laskowitz, "A Review of the Conceptual Status of Anxiety." This is a bold and critical analysis which commends itself to all thinking psychologists.

A REVIEW OF THE CONCEPTUAL STATUS OF ANXIETY

David Laskowitz

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There probably would be minimal dispute with the contention that if science is to be perpetuated it must have a socio-linguistic character, i.e., be susceptible of communication. Yet, the concept of anxiety, which is to occupy a pivotal position in personality theory, is nevertheless embedded in a matrix of semantic confusion. Contributing to the conceptual haze is the fact that definitions are too frequently a function of the phylogenetic level at which investigations are made, that they vary with respect to whether they incorporate either phenomenological or empirical criteria or are products of "neurologizing," and finally, that they are necessarily linked to the circumscribed area of competence of the researcher and are consequently refractory to intersubjective reduction. Thus, anxiety has alternately been conceptualized in terms of test variables, physiological and learning criteria, psychoanalytic hypotheses, etc. As a case in point, one has only to contrast Kurt Goldstein's formulation of anxiety as an essentially subjective experience of the organism to threat from a catastrophic situation (14, p. 295) with Mowrer's initial explication of anxiety as "a conditioned form of pain reaction" (30). (The latter investigator has subsequently evolved a "guilt-theory" of anxiety in which excessive repression of the superego, rather than libido, is posited as the precondition for anxiety.) Likewise, by no means is there agreement as to whether the anxiety-like reactions so frequently reported in animal studies are equivalent to their counterpart in humans. Thus, whereas investigators like H. S. Sullivan (24, p. 96) and Mowrer (24) had taken a negative view on this issue, Goldstein (14, pp. 301-302) has represented a faction that has taken an affirmative stand. Liddell (21), on the other hand, has adopted a more conciliatory position and has expounded his conviction that, though animals probably do not exhibit anxiety in the human

sense, they do manifest vigilance (cf. Pavlov's "investigatory reflex"), which can be considered an infrahuman analogue--if not homologue. In the latter connection, it is noteworthy that for Liddell, "vigilance" is not merely the "neural vigilance" employed in Head's linguistics, i.e., a state of high grade efficiency at some part of the nervous system (26, p. 272). In place of the latter formulation which stresses leakage of energy along newly formed pathways, Liddell proposes an alternative hypothesis in which vigilance derives from involvement of the entire psychobiological apparatus. The importance of organismic, in contradistinction to more circumscribed neurophysiologic considerations, is reflected in the empirical formula tentatively offered by Liddell, viz., that primitive vigilance minus social communication (i.e., a means of discharging energy) equals anxiety (21, p. 196).

The equivocal status of the concept of anxiety is further complicated when one attempts to cope with necessary ancillary problems, such as a need to distinguish between an imposing array of terms like anxiety, dread, tension, fear and panic, and the equally important task of determining whether a temporal or a cause and effect relationship obtains between them. Let us therefore examine some of the ways in which the above concepts have been manipulated. To begin with, many authors have been inclined to consider fear the more generic concept, with anxiety and panic representing derived phenomena, e. g., Miller and Dollard (27). In this connection, it might be recalled that John B. Watson posited a "two fear" theory of the new born infant, i.e., fear of loud sounds and fear deriving from loss of support, and that Szekely (39, p. 66) likewise makes mention of "archaic fears." In partial agreement with the latter thesis is Oskar Diethelm's explication of panic as a fear based on prolonged tension (9), and Emanuel Miller's dictum that "fear is the biological foundation of anxiety" (25). In contrast, it is instructive to compare the more extreme views reflected, on the one hand, in Sandor Rado's position that the term anxiety be discarded from technical usage in favor of fear which has greater heuristic value (33), with the Goldstein-Freud-Horney contention that it is anxiety that is actually primal, and that fear is essentially a derivative. The rationale for the latter

position is anchored in the proposition that fear presupposes a capacity to objectivate. It is therefore deemed logically consistent to assume that some degree of neurological and psychological maturation is a necessary precondition for fear to be activated. To heighten the confusion still further, one has merely to reflect upon Paul Hoch's assertion that anticipatory fear and anxiety are dynamically so close that they are virtually identical in some instances (15). This compares well with James Miller's suggestion that anxiety "is a signal flag which appears in the subjective experience of an individual when he is uncertain about the nature of the environment and therefore fears that it may be threatening" (26, p. 264).

Yet, the situation is by no means chaotic. Thus, though there is clearly disagreement as to the relative importance of the above concepts and their temporal relationships, there nevertheless is widespread acceptance of at least one basic difference--namely, the Kierkegaard and Heidegger notion that fear pertains to "somethingness," in opposition to anxiety which has no object. Of course, the preservation of the latter distinction is essentially a tribute to Freud's efforts in the realm of motivational theory. However, is it sufficient merely to state that with fear an object serves as an external referent whereas with anxiety the latter is lacking, or not apparent? What about the character of this referent? Jersild's work (16) on the unpredictability of fears in children attests to the empirical truism that an object which may evoke fear at one time, may be devoid of affective tone, or even positively accentuated at others. In fact, it is clinically observed that some people seem to derive satisfaction from exposing themselves to noxious stimulation. Yet, it will be recalled that pain is usually considered the necessary antecedent of fear. Antithetically, anxiety is seemingly always negatively toned. There is still another distinction between the two constructs that would probably be consonant with the view of most therapists. In fear, reassurance is possible by explaining that the environmental situation is actually not threatening, or that the possibility exists of overcoming the danger. However, as Goldstein (14, p. 293) has cogently pointed out, in anxiety states the

patient remains adamantly refractory to such reassurance. This is what Paul Tillich (40, p. 18) has reference to when he extols the power of the word, since through the media of verbalization the therapist can endeavor to effect a transformation from anxiety to fear. In Tillich's words, "fear can be overcome, anxiety can only be reduced" (40, p. 17).

Another difference between anxiety and fear which is posited by some investigators, but which nevertheless remains a moot point, lies in the physiological realm. For example, Noyes (32) believes the physiological reaction to fear and anxiety to be similar if not identical, and has his opinion bolstered by Waelsch's observations (41) that there are as yet no reliable bases for discriminating between the two phenomena with respect to specific biochemical variables, although hope is extended that a more thorough understanding of intermediary brain metabolism may furnish important clues. Wolf and Wolff (42), on the other hand, have dramatically pointed out that the two states energize different defense reactions, and that the activated mechanisms do have physiological correlates. Thus, in fear, there is frequently preparation for flight and, consequently, a related cessation of gastric activity, in keeping with Cannon's emergency thesis. However, in anxiety, where the external referent is unknown, and goal-directed behavior breaks down, there is an exacerbation of gastric activity. The above mentioned linkage between anxiety and heightened gastric involvement has also been convincingly demonstrated in the research on ulcer formation conducted by Mittleman, Wolff and Scharf (29, p. 58). Diethelm (8) is still another sponsor of the proposition that specific emotions have specific biochemical correlates. Examining the effects of blood samples obtained from both clinically tense and predominantly anxious individuals upon various isolated tissues, e.g., the duodenum and uterus of a rabbit, he discovered predictable reaction patterns. Thus, there was an adrenergic effect produced by the blood of subjects showing anxiety as a prominent emotion. In contrast, the blood obtained from tense individuals exhibited cholinergic effects. It is noteworthy that though both Diethelm and Wolf and Wolff are in accord about autonomic involvement with anxiety arousal, they disagree as to which division of the autonomic nervous system is activated. Perhaps the solution lies in Gellhorn's position (13) that both divisions

are inextricably involved, or in adopting Hans Selye's nomenclature (36) in which the two concepts, anxiety and fear, are coalesced into the more generic construct of stress -- especially since the latter has been defined in terms of well-worked out homeostatic mechanisms. Thus for Selye, stress is any condition that produces an "adaptation syndrome." This in turn is conceptualized as the reaction of the organism returning to a state of physiological equilibrium.

In view of the above discussion, it is small wonder that Maslow (23) recommended that authors append a subscript containing their initials to symbols and concepts used in order to insure that the specific meaning intended is grasped. In the same spirit, William Schoenfeld (35), subscribing to the maxim that "a word is no better in science than its clarity and unity of meaning," has no qualms about giving a completely descriptive and positivistic definition of anxiety which is devoid of what Reichenbach (34) called "surplus meaning," i.e., verbal terms which are not linked to external referents and must therefore remain conceptual fictions. For Schoenfeld (35, p. 74), anxiety is "the relationship between the presentation of S_1, after the $S_1 \rightarrow S_2$ operation has been performed, and certain measurable behavior changes" (where S_1 is an initially neutral stimulus, S_2 a noxious stimulus, and $\rightarrow$ indicates the passage of some time between the pairing of S_1 and S_2). By "measurable behavior changes," Schoenfeld explicitly means responses which will terminate S_1, since the latter stimulus has assumed aversive properties as a consequence of linkage with S_2.

The semantic stew is further flavored by the widespread, careless use of psychoanalytic lingo. As both Kris (18, p. 333) and Rado (33, p. 151) have ably pointed out, the multitude of loose translations from German to English have been "unfortunate." In the same vein, it could be added that the scientific palatability of analytic contributions often suffers from uncritical use of over-extended metaphors. If one considers theory to be in large part a system of language, the above difficulty can prove a formidable obstacle. Indeed, one has only to recall Whitehead's "fallacy of misplaced

concreteness," in which the mere giving of a name implies its concrete reality for some.

To return to the theme of this paper, what is the present conceptual outlook for anxiety? How are we to recognize its existence? Shall we put our faith in observational criteria? If so, will they be of the introspectional or inspectional variety? In short, shall we place our emphasis on inferential techniques of obtaining data about anxiety or on so-called "direct" experience? In essence, the above questions reduce themselves to an analysis of the operations that might be performed to bring the study of anxiety into the experimental fold. Brief mention of the background of this issue, beginning with Feigl's endorsement (1930) of the Bridgman doctrine (3), which was destined to sensitize psychologists to the operational method, might be appropriate at this point.

Having been launched with Feigl's sponsorship, the operational revolution in psychology gained impetus from Stevens' efforts (3) in disseminating Bridgman's views, in which he assumed the role of expositor. Bridgman, it will be recalled, had propounded that though no concept is without a "penumbra of uncertainty," every concept is nevertheless reducible to a set of operations. The rebuttal to the above canon was well presented in Bergmann and Spence's pointed query (1) of whether one does in fact arrive at two different concepts of length when one measures the length of a table and the distance to a star by two different operations, i.e., a ruler versus triangulation methods. Translating the above to the topic of this paper, one might ask whether failure-induced anxiety, as in the case of an ego-involved subject who is given insoluble anagrams to complete, is the same as task-induced anxiety, where the subject is prevented from functioning optimally because of distractions (e.g., electric shock, noise, verbal disparagement from the examiner). In fact, are task-induced anxieties evoked by two different operations functionally equivalent? For example, how might the anxiety induced by sustained distraction differ from the anxiety derived from pacing the subject too rapidly, i.e., demanding too much of a learner (20)? It would seem that what are

desperately needed are operations to establish the equivalency of other operations.

However, despite the questions seemingly left unanswered in an orthodox adherence to Bridgman's position, he nevertheless is quite capably supported by many theoreticians. As a case in point, a very prominent supporter is had in Herbert Feigl (3), who has subscribed to the doctrine that no statement has meaning apart from the methods employed to establish it. At first glance, the implication for the problem of anxiety, if one is a proponent of the Bridgman-Feigl approach, is that the various definitions of anxiety that have been formulated by diverse disciplines have no common denominator but are in actuality tapping different phenomena, i.e., there are different anxieties, anxiety$_1$, anxiety$_2$, etc. Fortunately, the dilemma was also averted by the contributions of members of the Vienna Circle--particularly Carnap (7). The latter, for example, advanced the notion of the "partial definition," which was deemed heuristic for scientific theory development. A "partial definition" has an advantage over the "complete definition" in that it does not irreconcilably limit the meaning of a term, but permits for expansion of the meaning as more understanding of the concept accrues (7). To cite Neal Miller's illustration (28, pp. 83—84), temperature may be defined in terms of expansion of a column of mercury. However, the definition has a partial character in that temperature can also be defined in terms of other phenomena, e.g., the electrical resistance of wire, the voltage produced by a thermocouple, the spectrum of emitted light, etc. Furthermore, protagonists of the Vienna Circle hold that these partial definitions will ultimately be coordinated so as to generate a unified science, with the proviso that language conventions be revamped so that the linguistics of the manifold disciplines can be made translatable into the language of science. (Hence, the coining of the word "physicalism" by Otto Neurath.)

In passing, perhaps we can profit from past attempts to conceptualize intelligence with the aid of factor analysis (cf. Spearman's two factor theory with Thorndike's multi-

modal theory of intelligence). Perhaps, here, too, to use Spearman's linguistics, different techniques of inducing anxiety merely generate "s" factors which obscure and complicate the isolation of a "g" element, if indeed, one exists.

Again, returning to the above implications for this paper, are we ready to merge the manifold conceptual contributions of anxiety into an intersubjective theory? The historical popularity of the latter goal has had, of course, multifarious antecedents, ranging from Leibnitz's efforts (1666) to logicalize human thinking to Mach's teaching (1886) that experience is a common subject matter of all the sciences (3), to mention but two, up to the present day. Thus we find analysts like Rado endeavoring to combine the work of Freud with that of Walter Cannon. In a similar vein, we have theoreticians like Urie Bronfenbrenner (4) striving for what in Kurt Lewin's system would have been termed an homogenization of psychological schools. Thus Bronfenbrenner (4) searches, with some success, for common denominators among the doctrines of Lewin, Freud, Sullivan, Rank and McDougall. Even Freud himself considered the current lack of correlation between the hypothetical functions attributed to admittedly metapsychological structures (e.g., the id, ego and super-ego) and neurological facts as merely provisional.

However, back to the original question. Is the time ripe for a synthesis? Was Brunswik (6) correct when he referred to psychological research as "a pattern of fragments?" Or, would it be better if the various disciplines continued their independent investigations and built up their respective areas of knowledge before attempting an integration? Certainly, there would be little dispute with Marx's view (22) that there is ample room for both approaches, and that the two approaches are to be considered complementary rather than mutually exclusive. Yet, what is seemingly needed is more vertical development, more of the constructive type explanation of concepts. Once that is accomplished, energy can be expended in striving for horizontal extension of information. with concomitant use of reductive-type explanation.

In our endeavor to investigate the type of construct that "anxiety" typifies let us cursorily examine the thinking on this matter. Historically, at least two basic types of propositions have evolved: (a) a formal type consisting of terms that have no empirical reference, and (b) an empirical species in which an array of terms are coordinated with observable events (22). This "Schlickian" dichotomy has been precursor to the development of what Kenneth Spence (37) delineates as four kinds of theoretical constructs, viz., animistic (Schopenhauer's "will," Bergson's "elan vital," Freud's "libido"), neurophysiologic, response inferred (anxiety indicators as revealed by psychological test signs, clinical observation, etc.) and intervening variables. Advocates of the Hull-Spence-Neal Miller school have a theoretical bias in favor of the latter type. Thus, to them, the term anxiety should ideally be given an intervening variable status in the strictest Hullian-Tolmanian tradition, i.e., it should be linked to observable antecedents and consequents. However, a dilemma inheres in the fact that behavior has an essentially open system character, and that consequently, a given psychological state can be reached through diverse antecedents. Conversely, the same antecedent may produce variations in consequences at different times. Apparently then, there would seem to be a need for classifying the manifold preconditions in a somewhat botanical manner, and then attempting to coordinate them with behavioral changes in a meaningful way. If this supposition is correct, the immediate prospect of developing the above type of complex law in a subject matter which is still very much in its latency stage does not seem very bright. Yet, this should not be construed as a stricture of constructs having 'intervening variable' properties. Quite the contrary, there should be continued exploration of their potential utility.

At this point let us examine Freud's thinking (12) on anxiety from an operational perspective. It was Freud's early contention (deriving from his formulations of 1894) that sexual inhibition through neurotic difficulties produced sexual frustration. The latter subsequently became physiologically converted to anxiety. The weakness of this early formulation was twofold. To begin with, it presupposed

neurotic structures without adequately explaining their genesis. Secondly, there was a need to analyze the process whereby frustrated sexuality became physiologically transmuted into anxiety. Since such an analysis would ideally have had to involve what Carnap has called "object sentences" (i.e., sentences using terms that are linked to observables), Freud's early formulation was probably experimentally of an indeterminate character. In contrast, the later modification of his thinking (1923) has shown greater promise. In the revised version anxiety has been conceptualized as a danger signal, and as such calls forth defense mechanisms to either terminate or reduce the mobilized anxiety. Thus, coping mechanisms such as repression could be operationally translated to circumscribed adaptive reaction patterns, e.g., associative or perceptual blocking as measured in terms of reaction time, or rage reactions when the tenuous equilibrium established by "repression" is threatened (cf. Goldstein's "catastrophic reactions"). As a case in point one has only to consider the experimentations on what Bruner and Postman (5) have dubbed "perceptual defense," i.e., an elevated recognition threshold for emotionally-toned percepts. Similarly, there have been investigations launched into an area that Lazarus and McCleary (19) have called "subception." These have involved experiments in which affectively-laden nonsense syllables are tachistoscopically presented at fast exposure rates, thereby cutting down on the stimulus information available to the subject. The result has been an inability to recognize the stimulus. Yet, it has been demonstrated that though there may be no cognitive recognition, there is often autonomic recognition as measured by such physiological indices as PGR.

Returning to Freud's system, the trend has been to give Freud's notion of anxiety an intervening variable status, thereby linking the concept to testable antecedents. For example, if one conceptualizes the idea of birth trauma as a physiological crisis characterized by such emergency reactions as rapid heart beat, difficulty in breathing, and diarrhea, all deriving from temporary asphyxiation, then one has a basis for measuring the intensity of the trauma and appraising its relationship to posited sequelae. Else

Frenkel-Brunswik (11, p. 394) furnishes another interesting example of providing testable antecedents in her assertion that perceptually ambiguous situations will mobilize energy toward rapid perceptual closure in a rigid individual, ostensibly because of the latter's inability to tolerate equivocality. This inability to remain exposed to perceptually ambiguous stimuli is explained in terms of the anxiety-generating properties of poorly defined visual fields, which endanger the intellectual defenses of inflexible individuals. However, one could readily establish if there indeed were a systematic relationship between stimulus impoverishment and anxiety, e.g., as measured by physiologic indices.

Up to now, the discussion has centered about inferring the existence of anxiety by an analysis of stimulus operations and response properties. The former are reflected in the two techniques of provoking anxiety, i.e., by inducing feelings of failure, and by manipulating working conditions so that the demands of the task can not be satisfied. The latter, on the other hand, have in part been illustrated in the reliance upon such syndromal clues as tachycardia, spasticity of the colon, pylorospasm, behavioral disorganization, etc., all of which are thought of as significant concomitants of anxiety. Still another approach has centered about the recognition of anxiety from an inspection of its cue and drive properties. For example, could not the amount of anxiety be deduced from the ease with which conditioning is effected, or from the extent of resistance to extinction? Likewise, could not the experimental data available on stimulus generalization be of help in conjecturing as to the facility with which the hypothesized signal properties will spread? It is exactly this type of orientation to personality theory that is espoused by Miller and Dollard (27).

The treatment of anxiety from the perspective of a motivational system holds promise of presenting valuable data. That anxiety has both directive and energizing aspects is relatively non-controversial. Clinical and experimental experience provides ample instances of both. The directive aspect makes for expectations that the anxious organism will

move toward goals that will terminate anxiety. The energizing properties are likewise frequently relied upon by the therapist who, for example, strives to induce anxiety in a complacent client, in order to heighten his productivity or to undermine unadaptive defenses that have become overly stabilized. Here, however, another problem presents itself. Beyond a certain range of intensity, anxiety build-up ceases to have a facilitative effect and may become disruptive. It is for this reason that it would be of considerable value to have data on what constitutes optimal non-disruptive anxiety.

In the literature there is evidence of attempting to explain anxiety by positing hypotheses about its purpose, the most popular position being that it insures the organism of survival by instigating appropriate avoidance behavior (e.g., Cannon, Mowrer, etc.). The rationale underlying this type of "explanation," however, remains quite controversial. For example, if we can temporarily digress and examine the biological model of theory construction, it is noteworthy that biologists have traditionally sought to understand physiological structures by posing "why" questions, e.g., why is it that a specific structure is necessary for an organism's survival? Curiously, this line of query usually helped the investigator to arrive at "what" and "how" explanations. For example, the above question raises conjectures as to what would happen if the structure in question were extirpated, coagulated, over-stimulated, etc. A corollary query might pertain to how the modification of the above structure might affect the functioning of neighboring structures, and ultimately the behavior of the organism. In short, as Krech and Klein (17, p. 11) have ably pointed out, "why" questions posed in the Aristotelian tradition can be very valuable in that they help focus the problem and provide a basis for arriving at "how" and "what" explanations. This is quite different from formulating a teleologically-toned "why" answer which implies an a priori conviction in a primal goal toward which behavior is directed.

There is still another objection to explaining the concept of anxiety on the basis of purpose, or for that matter

explaining it on any basis. This is the Bergmann (1) position that concepts in themselves are not supposed to be explanatory. Rather, it is only when laws and theories are formulated in terms of these concepts that explanatory powers are engendered.

In passing, the attempted understanding of anxiety in terms of its survival value provides an interesting paradox. It is medically known that people very literally die of anxiety, e.g., they become so anxious that there is heart failure (8 , p. 107). Curiously, it is Cannon himself who writes at considerable length about the detrimental effect of sustained sympathetic stimulation that is occasioned by Voodoo rituals. Similarly, Mira (24, p. 69) tells of the "malignant anxiety" which was perceived among soldiers in the Spanish Civil War (1936-39), and which was responsible for fatalities in the absence of physical trauma.

When then is the future status of this enigmatic construct referred to as "anxiety?" Whatever it is, shall it be conceived as being a basically constructive or destructive force? Mowrer (31) contests the view that anxiety is a noxious phenomena which should be done away with in the interest of sound mental hygiene. He would remind theoreticians of Freud's thesis that culture consists of sublimated anxiety, and that anxiety, when properly harnessed, can be a very constructive force. Certainly, a small amount is needed since it can impel to action and can counteract the lethargy that might otherwise accrue with complacency. Yet, whereas the dynamogenic features of anxiety in small quantities are doubtless desirable, psychological studies have amply demonstrated the adverse effects of intense anxiety. The consequences run the gamut of behavioral reactions ranging from stereotopy, primitivization and impaired stimulus evaluation, to frantic, agitated behavior that is devoid of goal orientation. Another question which comes to the fore relates to the manner in which the problem of latent anxiety will be investigated. Its prognostic importance from a psychopathological point of view is readily apparent.

In conclusion, although Felix Deutch's position (10, p. 80) that "every disease is an anxiety disease" is probably an overstatement of the problem, the dearth of information pertaining to the conceptual status of anxiety warrants renewed efforts if this hiatus in personality theory is to be bridged.

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VISION WITH SPATIAL INVERSION: A FOLLOW-UP STUDY[1]

Fred W. Snyder and Charles W. Snyder[2]

The Menninger Foundation

Three earlier reports of an extended study of spatial inversion (2, 3, 4) showed the relative facility with which a human adult S could reorient to an inverted spatial field. The study was designed to answer these questions: How are visuo-motor coordinations modified when the visual field is inverted? What features of performance during the period of wearing the inverting lenses and after their removal are related to visuo-motor habits developed before the experiment or in preceding experimental periods?

The original experiment was divided into three periods: practice without inverting lenses, practice with inverting lenses, and continued practice after their removal. In each of these three periods, continuous training in card sorting, a manipulation test, a pegboard test, and a mirror tracing task were carried out. Observations were made of S's walking, language activities, visuo-motor activities, auditory-visual conflicts, and other behaviors. S kept a diary of his observations of daily happenings in the laboratory and in daily living. The results (curves labeled first experiment in Figures 1, 2, 3, and 4) indicated that adjustments to seeing an inverted visual field occur relatively rapidly. For most experimental tasks S was making the lowest time scores of the experiment toward the end of the period in which the inverting lenses were worn. For compa-

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1. The authors wish to express their appreciation to Dr. N. H. Pronko and also to Drs. David T. Herman, Robert R. Holt, George S. Klein, Gardner Murphy, and James E. Simpson for continued helpful suggestions.

2. Formerly at the University of Wichita.

rable proficiency in some of the extra-experimental activities, such as walking down stairs, a period longer than 30 days (or more practice during this period) would have been necessary.

The work of Kohler (1) indicates that extended wearing of a device that inverts but does not reverse right and left produces "phenomenal righting of the visual world." That is, S reports that what he sees looks right-side-up or "normal."

In the present experiment the following questions were asked: What will happen to the learning curves if S again wears inverting lenses after a long period of time? Will the curves of learning for the repeated tasks begin at the same level as in the first experiment? Will the time curves decrease at the same rate as before? And how will the learning curves compare with the earlier curves near the end of the experiment?

METHOD

S's 30-day experience with the laboratory tasks nearly two years before made the present experiment essentially a relearning situation. The binocular inverting lens system and headgear were identical with those used for the original experiment (4, p. 46 ff). The lenses had a unit-power 20-degree field with 180-degree rotation of the field. The 180-degree rotation provided right-left reversal and up-down reversal of the spatial field for S.

It should be brought out that several factors that were not present in the first experiment were introduced in the second. S (first author of present paper) did not wear the lenses continuously, but he wore them only during the daily one-hour sessions; further, the lenses were frequently removed between tasks. Also, there were preceding sessions without inverting lenses in the first experiment, but there were none in the second experiment.

Card sorting, Minnesota Rate of Manipulation, Purdue Pegboard, and mirror-tracing were the four experimental

tasks relearned. S completed 5 trials per session in each of these experimental tasks. The time was recorded to the nearest second.

The first task utilized a modified Jastrow card-sorting box, similar to that designed by Ewert. This slanting box contained nine compartments (three rows and three columns), each of which was verbally assigned a number from one to nine in order from left to right and top to bottom. The sorting deck consisted of 144 standard playing cards, 16 each of the number one (or ace) through number nine card. One-half of the sorting deck was placed back-side-up in S's left hand (he is normally left handed) and at E's signal, "Ready..... go," he shifted the cards to his right hand and began sorting them with his left hand into their respective compartments. The other half of the deck was placed on the edge of the table at S's left and was picked up and sorted with his left hand after he finished the first half. After each trial E sorted the cards into 16 identical stacks, counted the errors, and shuffled the deck thoroughly.

The second task utilized the Minnesota Rate of Manipulation Test. This test consisted of 60 cylindrical blocks that S was required to turn over in a planned sequence with his left hand. First, S turned the block over in the upper left position. He turned over the remaining three blocks in this column in an order from top to bottom. Then, he moved to the right, to the second column, and worked up. S continued working in an alternating direction until all the blocks were turned.

The third task employed 22 pegs, washers, and holes of the Purdue Pegboard. On the signal, "Go," S picked up a peg and a washer, inserted the peg in the washer and, with his right hand, dropped the peg in the hole nearest him on the left. S picked up and put together another peg and washer and placed the peg in the hole nearest him on the right. He filled the next hole above on the right. Then, he placed a peg with washer in the hole on the left. S continued, alternating in this fashion, until the last hole was filled.

The fourth task, mirror-tracing, employed a notched-tooth star pattern, stylus, mirror, and shield. A magnetic counter registered an error each time the stylus touched either side of the pattern. A chronoscope was automatically started when the stylus was moved off the starting point, and was stopped when the stylus touched the stopping point.

After each session the mean time for the 5 trials on each task was obtained. The learning curves from the first experiment were also shown on each graph.

At the end of each session S was informed both of his performance for that session, as well as how this compared to the original learning of the tasks in the previous experiment.

RESULTS AND DISCUSSION

A consideration of Figures 1, 2, 3, and 4 indicates that each curve of time in the present experiment starts lower and follows the trend of each comparable time curve in the first experiment. This correspondence of learning curves is greatest for the Purdue Pegboard Test (Figure 3).

Although an error score was obtained for card sorting and mirror-tracing, these data are not presented since it would greatly complicate the graphic presentation. The only significant result for errors was that the error curve for card sorting in the present experiment was at a notably higher level than the corresponding curve in the first experiment. Although S was aware of the results, he reported that his attention was primarily on a reduction in time. Perhaps the errors could have been brought down at the expense of increased time. No test of this possibility was made in the present experiment.

The learning curves of time for each task (1) before, (2) during, and (3) after the period of inversion on the first experiment, as well as the curves for the present experiment, appear to be nothing more than standard learning

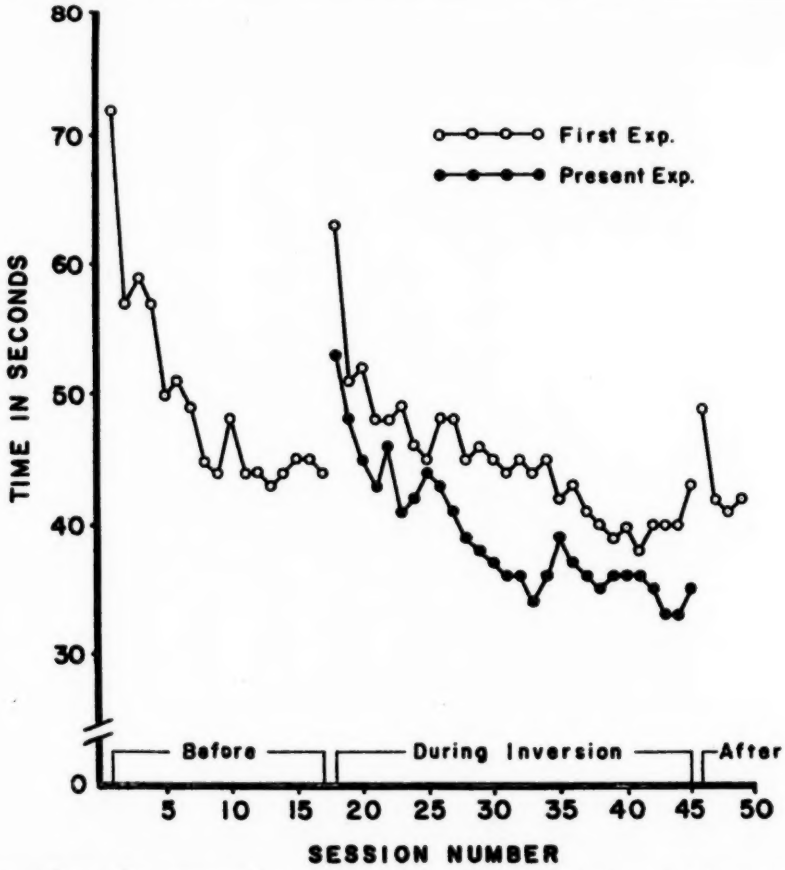

Figure 1. Average time in seconds per each five trial of Minnesota Rate of Manipulation Test.

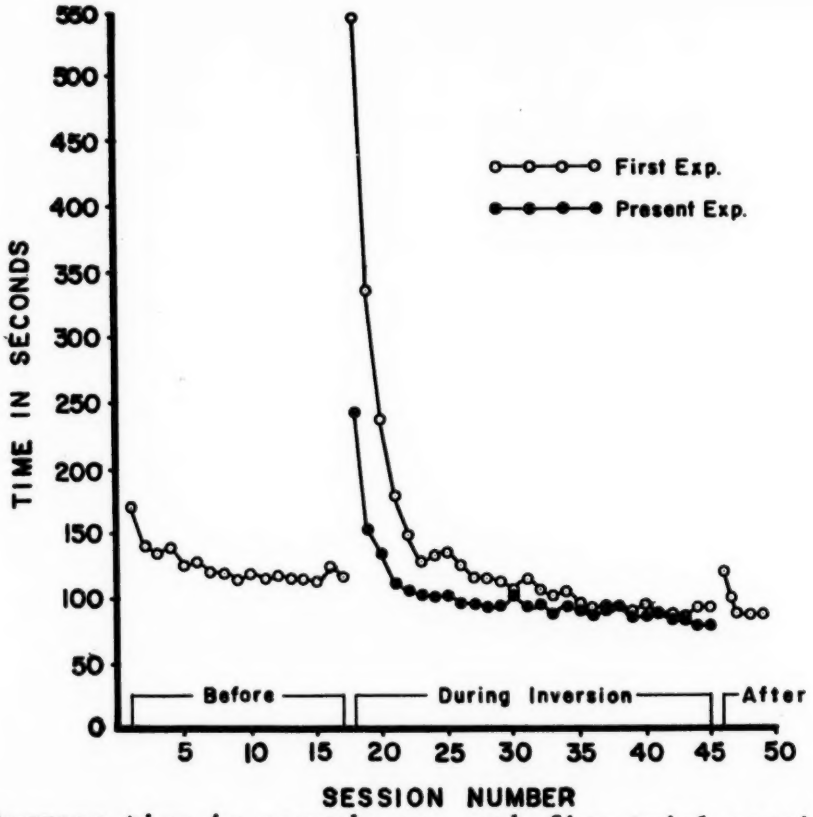

Figure 2. Average time in seconds per each five-trial session of card sortings.

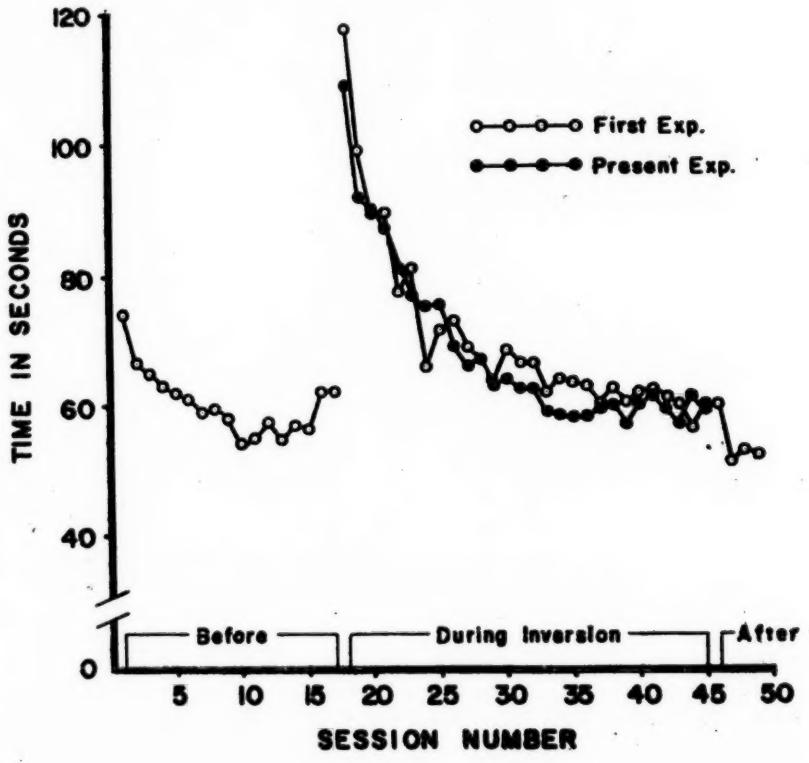

Figure 3. Average time in seconds per each five-trial session of Perdue Pegboard Test.

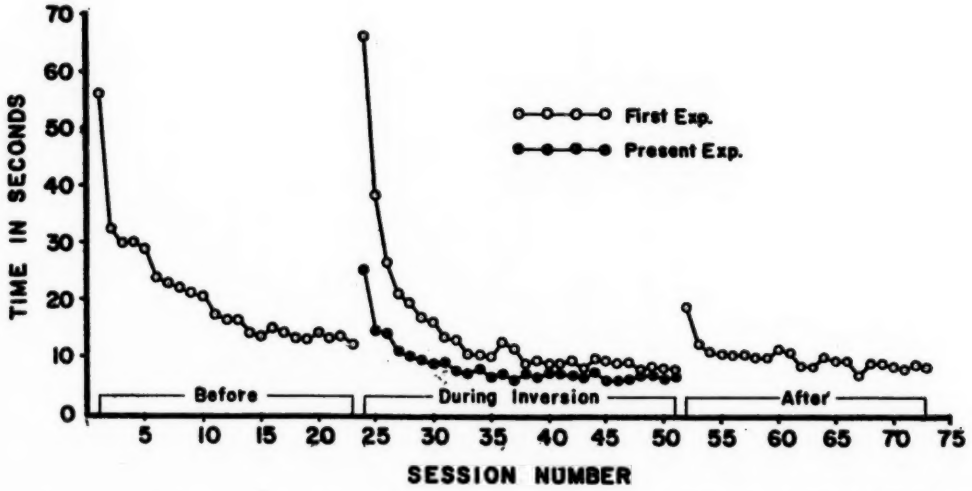

Figure 4. Average time in seconds per each five-trial session of mirror tracings.

curves. Now, if we take the complete learning curve (during all periods) for any task from the present experiment and plot it graphically as a continuation of the complete learning curve for the same task of the first experiment, and if disruptions of learning resulting from the sudden changes in experimental conditions are disregarded, it is apparent that the sections form one continuous learning curve. This phenomenon suggests a factor of modifiability of visuo-motor reactions in this learning situation. S continued to learn under such varied conditions as these: The visual field is suddenly inverted; it is suddenly righted after 30 days; and finally the visual field is reinverted after almost 2 years. Although the variable of inversion did produce reversals and conflicts in visuo-motor actions, S quickly adapted to each new set of conditions by altering his perceptual behavior in a specific manner.

The continuity of the time learning curves from the first to the present experiment may in part be due to the fact that S did not perform the specific experimental tasks under any condition during the 2-year period between experiments. Therefore, S's skill at these isolated tasks was little disrupted by the intervening everyday activities.

There is evidence in E's observations and S's reports during the inversion period to indicate that occasionally S responded blindly, according to the habitual perceptual schema, before recognizing his errors. The learning curves do not indicate this because S was already acquainted with the features of inversion even before the inversion period in the first experiment. Thus, he was quite aware of the conflict and began delaying most responses at the beginning of both inversion periods. However S occasionally completed inappropriate responses during the inversion period, especially at the beginning; e.g., a left-right reversal of placing a number four card into the number six compartment and a number six card into the number four compartment.

It appears clear that perceptual orientations for appropriate responses with normal and inverted visual fields are in direct conflict. S in the inversion setting at first

attempts to continue in the familiar pattern but very quickly recognizes the inappropriateness of his responses; e.g., he moves his hand in a direction he believes to be toward the object and discovers that his hand is actually moving farther away from the object. Each visuo-motor reaction made automatically, in the habitual way, must be corrected before the proper response can be completed. The result of this conflict is apparent from the very first trials in the new setting. S must delay all responses in order to inhibit each habitual movement and substitute the new perception which results in the proper movement.

A further examination of the learning curves suggests that two years of normal visuo-motor reactions between experiments has only a slight and momentary transfer effect in relation to inhibiting the progress of the tasks performed with the inverting lenses. Perhaps it would be possible for Ss to learn to shift rapidly from normal to inverted vision after training to do so. An experiment could be designed to include experimental tasks which the same S learns to do with both normal and inverted vision to test this hypothesis. As previously mentioned, S in the present experiment frequently interspersed normal vision between tasks during the experimental sessions with inverted vision with no apparent negative transfer conflict. It would be interesting to compare these results with an experiment using Ss naive to the inversion situation. No doubt the transfer effects would be greater, and perhaps there would be an opportunity to study variability in behavior at the beginning of the inversion period.

SUMMARY

The spatial inversion period of an earlier experiment was repeated under similar conditions two years later. The questions asked were: What will happen to the learning curves? Will the curves of learning for the repeated tasks begin at the same level, decrease at the same rate as before, and how will they compare with the earlier curves near the end of the experiment? The binocular lens system and head-

gear were the ones used for the original experiment. The four visuo-motor tasks performed were identical to the ones used earlier. However, the lenses were not continuously worn as in the first experiment.

Each curve of time in the present experiment started lower and followed the trend of each comparable curve in the first experiment. The results of both experiments suggest that visuo-motor reactions are modified, under the varied conditions of these experiments, to reduce conflicts and produce a lower time score. The results also show that occasionally S responded blindly in the inversion period, according to the habitual perceptual schema. Finally, two years of normal visuo-motor reactions between experiments had apparently only a slight and momentary negative transfer effect on the progress of the tasks performed with the inverting lenses by S of this experiment.

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PERSPECTIVES IN PSYCHOLOGY.....

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An introductory note:

When the editor of this journal broached the idea of a regular contribution of perspectives on the current psychological scene, the writer responded with a strong "approach" function. It appeared that a medium for evaluation and comment from an interbehavioral frame of reference on a somewhat freer level than the regular journal article could meet a useful and perhaps productive function for the subscribers of this journal. The writer committed himself to trying this for the journal. Since that time the writer's interbehaviors with the idea developed into an "approach plus avoidance" stage, but not because the idea lacks merit. It was rather that it seemed presumptuous for any one person to carry it alone. The chief variable that helped the writer get off the "point of intersection of the axes" of approach and avoidance was the editor's agreement with the proposal that these pages (currently limited to four) also be freely open to the subscribers for their ideas on what appears provocative from the interbehavioral point of view. An invitation is hereby issued. What has been on your _____?

The Ames Demonstrations and Transactionalism

The rather considerable attention given in the recent literature to the Ames demonstrations in visual perception is, we would suggest, of particular interest to interbehaviorists. In addition to their behavior demonstration value, which is not insignificant, pertinent experimental research making use of these stimulus situations has been published, as well as a number of theoretical articles, by Ames, Cantril, Kilpatrick, Ittelson and their associates. The framework of interpretation of these papers is called "transactional," and is presumed to derive from the transactional concepts of

Dewey and Bentley's Knowing and the Known (4). It is our view that both the behaviors made available with the Ames demonstrations as well as the interpretative constructs applied to these behaviors within the transactional framework warrant the careful attention of interbehaviorists. A limited set of pertinent references would be Ittelson's (5) admirable manual describing the construction as well as the behavioral functions of the equipment, Ames' (2) interpretative manual, Kilpatrick's (9) collection of relevant experimental and theoretical papers, and Ittelson & Cantril's (6) paper on perception. A brief description and critical assessment can be found in Allport's (1) volume on theories in perception.

Some of us at Wichita are making use of the Ames equipment. In our recently expanded laboratory facilities, made possible in part by a National Science Foundation grant for the improvement of our undergraduate program, we have reproduced a number of the Ames pieces for both teaching and research purposes. Some have been constructed with modification of the original design. Dr. Hadley Cantril, who is in charge of the excellent collection of equipment at the Perception Demonstration Center in Princeton, very generously contributed several pieces and parts. We are most grateful to him.

Photographs and descriptions of some of the equipment, such as the trapezoidal windows, string chairs, overlay, and distortion room are available in the general psychology literature and so will not be described here. Essentially, the equipment consists of objects, model size rooms, and situations in which objects and setting components are arranged or can be manipulated to produce extremely effective illusions. (It is, incidentally, quite difficult to make effective verbal descriptions of the perceivings without having the concrete stimulus object and response referents present.) One of the chief significances of the demonstrations, in our view, is that they help to make the operation of stimulus functions strikingly demonstrable in observation of both one's own behaviors as well as the behaviors of others. Simple manipulations of the objects or

slight shifts in the position of the observer result in marked changes in the way in which the objects and/or settings are perceived. The strength with which particular stimulus functions operate despite the observer's knowledge of the stimulus conditions, based on prior different interbehaviors with them, is impressive. Several of the pieces make changes in, and the evolving of, stimulus-response functions readily observable as a function of manipulative interactions by the observer. No presumption is made that the Ames pieces are unique for demonstrating the operation of stimulus functions, but it is suggested that they have particular merits for research in the operation of stimulus-response functions. Research work in this area is in process in Wichita.

In the matter of theory development the interbehaviorist may find himself attracted to the transactional orientation on the basis of a presumed affinity with the interbehavioral orientation as types of "integrated-field" (8) development. If he examines the Ittelson and Cantril (6) paper he is likely to be disappointed. This is not to say that the paper is without merit for descriptive constructs regarding perceivings. The interbehaviorist, we would suggest, will find a number of points in Ittelson and Cantril that are compatible with the interbehavioral orientation since Kantor's treatment of perceivings in 1924 (7): that stimuli operate within the behavioral event as "significances," that there aren't unique one-to-one relationships between energy impingments and perceiving, that all parts of the (behavior) situation enter into it as active participants, that perceivings are predictive (semi-implicit) for other actions, and that the development of perception is primarily the learning of significances. Yet the total Ittelson and Cantril treatment, promising though it is, falls short of the integrated-field development. This, we would suggest, may be due to the failure to carry through with the transactional premises. The organism becomes weighted with variables better left as components of the transactions. The Ittelson and Cantril concept of perception as "externalization" has an uncomfortable overtone about it. The necessity for "awareness" as a component of perceiving and the place of "purpose" as a determiner are not at all clear. Just what is meant by the weighted average of past

registered assumptions which is accomplished through a largely unconscious process and brought to bear as an influence on present perceptions requires refinement. Perhaps a useful suggestion for the further development of the transactional orientation would be for the transactionalists to examine, if they have overlooked it, Bentley's (3) treatment of Kantor in "Behavior Knowledge Fact." It may be found useful in the effort to avoid endowing the organism with what seems to be close to the classical omnipotent consciousness for the understanding of perception.

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THE PROPER STUDY OF MANKIND IS RATIONAL MAN: A BOOK REVIEW

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A study of thinking. By J. S. Bruner, J. J. Goodnow & G. Austin. New York: Wiley, 1956. $5.50.

A Study of Thinking is nothing of the sort. It is readable, interesting, fresh and original; but it is not a book in the usual sense of the term, nor does it mention the word "thinking" in the text--much less consider this broad and ill-defined area. Rather, the book is composed of two distinct sections: one concerned with the work of the Harvard Cognition Project on concept attainment, and the other a long appendix by Roger Brown entitled "Language and Categories," which is totally unrelated to the rest of the text.

On the second page of the preface (p. viii) the authors indirectly admit to the lack of fit of their title by announcing: "We have sought to describe and in a small measure to explain what happens when an intelligent human being seeks to sort the environment into significant classes of events so that he may end by treating discriminably different things as equivalents." On the last page (p. 246) they explain the rationale for calling their study of concept attainment A Study of Thinking. "...virtually all cognitive activity involves and is dependent upon the process of categorizing." By this line of reasoning A Study of Lexicography, A Study of Taxonomy, or A Study of Studying would be equally justifiable titles.

The study of categorizing begins with a consideration of kinds of categories, the process of categorizing, and the variables which affect it. The authors' approach to the process of concept attainment laudably bypasses philosophical questions on the nature of concepts and emphasizes the

subject's behavior in classifying. Their studies have been designed to make as much of this behavior observable as possible. The language in which the subject's behavior is described and interpreted borrows from the stylish vocabularies of game and decision theory, and, to a certain extent, of functionalist theories of perception (e.g., Brunswik). The resulting analysis of classifying behavior treats the environment as a source of information; i.e., objects and events are characterized by properties or "attributes." In order to determine which of these attributes are criterial for classification and which are not, the subject adopts a "strategy"--a systematic procedure for organizing information and testing hypotheses concerning it. The authors emphasize that the "strategy" need not be consciously recognized or rationally instituted by the subject. Unfortunately, although the concept of strategy plays an important role in the formulation and interpretation of the experiments reported, it is never rigorously defined and systematically related to other constructs, such as "hypothesis," nor is it consistently used. At times the term strategy seems to be used simply as a shorthand description of what the subject is doing; at other times it is invoked as an explanation for what the subject is doing; and at still other times it is used (preceded by the qualifying adjective "ideal") to provide a logical analysis of what the subject should be doing. In this last usage the concept of ideal strategy provides a theoretical model yielding predictions for statistical comparison with the subject's observed behavior--a possibility which is not fully utilized. As to the general utility of the concept of strategy and of the general approach, consideration of this question will be postponed for a look at the experimental portions of the book.

In this reviewer's opinion the experimental studies reported constitute the most original, interesting, and important part of the book. Unfortunately, the presentation and interpretation of experimental results is unforgiveably incomplete. Although a 2 X 2 factorial design is utilized in most of the experiments, not once does an F ratio appear; interaction effects are never mentioned. Although tables of frequencies or percents are provided, the reader is rarely

informed of the N on which the numbers are based, or whether they derive from raw frequencies, means, or medians. So far as the reader can determine, all decisions concerning the relative effect of a given variable are based solely upon inspection of these ambiguous numbers. The cynic might even suspect that some of the differences so triumphantly proclaimed are not statistically significant. Presumably, the appropriate statistical tests were performed and only significant differences reported, but these details are omitted in order to address a larger audience than experimental psychologists. In view of the limited scope of the book one wonders how long the larger audience will listen.

The authors' lack of completeness in reporting and interpreting the experimental data is especially unfortunate since it detracts from what appears to be a coherent and ingenious experimental program. There has been a consistent attempt to get away from the traditional paradigm in which the subject must learn to classify meaningless material with respect to a single perfectly valid cue, often without understanding of the nature of the task. In all the Harvard studies the subject was fully informed as to the nature of the task, the type of concept to be attained, and the relevant attributes to be considered; nonsense syllable labels were not used. The dependent variable measures employed include the order and type of instances selected for test, or the order and type of hypotheses tested (S was instructed to verbalize), in addition to neater measures such as number of trials to criterion, relative frequency of each class of categorizing response, etc. Although it is questionable just what is being measured in the recording of verbalized hypotheses, for example, the dependent variables reflect at least a start in the direction of studying observable behavior in relation to the situation in which it occurs. In their selection of independent variables the authors often seem to be prompted by a desire to approximate "real life" conditions, despite the fact that their espousal of a model-building approach relieves them of the need to do so. Perhaps their most successful sally into "real life" conditions occurs in the chapter on categorizing with probabilistic cues, in which they tackle the problem of how subjects go about categorizing

in a situation where only cues of less than perfect validity are available. Clearly, a great many methodological assumptions underly the choices of independent and dependent variables and of procedures employed; this reviewer wishes that an elaboration of some of those assumptions had been included. The last part of the final chapter contains an excellent -- but all too brief -- consideration of a few of these methodological questions.

The utility of the concept of strategy and of the general decision-making model remain to be considered; at this point the value seems to be largely heuristic. In this reviewer's opinion, little is accomplished by their introduction which could not have been accomplished even more simply and successfully without them. On the other hand, for all of Bruner's disavowals of them, ancient ghosts continue to haunt his metasystem. The notion of the rational subject who surveys the situation and adopts, somehow, a strategy which will enable him to minimize risk and effort while maximizing the utilization of information smacks of the Utilitarians' rational man. Of course, Bruner may chose to investigate the extent to which a rational-man model describes the categorizing behavior of subjects if he wishes. However, even if one accepts his sanguine evaluation of the accuracy of the model, he is still faced with questions as to its generality (a) for a more heterogeneous subject population than Harvard students, and (b) for a larger class of behaviors than the process of learning established categories. I have a hunch what the answer to both questions will be.

Admittedly, it is easy for a reviewer to play the game of "if-I-were-writing-this-book;" but if I were writing this book, I wouldn't. As I have suggested, there is little justification for slapping the contents between hard covers and retailing the resulting assemblage at $5.50. The 65 page appendix by Brown is a hodge-podge ranging from a brief introduction to linguistic theory to a brief historical survey of the "generic mental image" notion. No attempt has been made to relate it to the rest of the text (just as well; the attempt is doomed to failure). The rest of the text inter-

mingles speculation and discussion with experimental results, to the benefit of neither. Since the authors have considered both a limited problem area and very little experimental work except that of their own project, their plunge into print should most appropriately have been in the form of a monograph. In choosing the book form they appear to be parlaying into a mountain what is presently a novel and promising molehill.

QUARTERLY

COMMENTARY

PROFESSOR McCLELLAND AND THE PSYCHOLOGY OF MENTAL CONTENT

The development of psychological science, like all other human enterprises, is an historical process. Whether we attend to the investigative techniques of the psychologist, to the selection he makes of events for study, or to the character of the constructs he elaborates, there is no aspect of psychological work that can be intelligently understood without reference to the cultural circumstances in which it is performed. Not infrequently, these circumstances are powerfully anti-scientific in character. They are, as J. R. Kantor describes them in "The Logic of Modern Science," "cultural barriers to a scientific psychology." Unless the psychologist is unceasingly and knowingly vigilant, their influence upon his work may become so severe as to make the objective analysis of behavior a virtually impossible task.

It is in the period extending from the post-Aristotelian phase of Classical civilization to the close of the eighteenth century in the modern world that one finds the most instructive examples of the crippling effect of adverse cultural influences on the growth of psychology as a science. During this time, genuine contributions to a scientific psychology were rare indeed. It was a period in the history of our discipline that may properly be called the "bleak age." The splendid beginnings made by the early Greeks toward a completely naturalistic conception of behavior were buried under the dead weight of a progressively spiritized psychology. Dualism replaced objectivism and the new mentalistic constructs that developed within this framework ultimately hardened into a tough, deeply-rooted tradition. For almost two thousand years mentalism dominated the development of psychology.

Traditions die slowly; so do traditional ways of thinking. Although no longer the powerful and all-pervasive doctrine of earlier times, mentalism still persists in many forms as a cultural barrier to a science of behavior. Disavow it though we will, in psychology as elsewhere, we tend to be as the poet Cowper described us:

> "The slaves of custom and established mode,
> With pack-horse constancy we keep the road
> Crooked or straight, through quags or thorny dells
> True to the jingling of our leaders' bells."

The obstinate endurance of mentalism as an approach to behavior is well illustrated by a recent article written by Professor David C. McClelland of Wesleyan University. The article, originally a paper delivered at the Fourteenth International Congress of Psychology in Montreal, is titled "The Psychology of Mental Content Reconsidered." It appears in the July, 1955 issue of the Psychological Review.

Professor McClelland's argument is easily summarized. "Psychologists," he begins, "used to be interested in what went on in people's heads," that is, in mental content. With the development of modern scientific psychology this interest gave way to a concern for "'process variables.'" Psychologists ceased to be interested in "what people thought, what they perceived, what they learned, etc." What was of interest were the "laws which governed the process of perceiving, learning, etc."

With the rise of the projective testing movement, writes Professor McClelland, this attitude began to change. Interest in a psychology of mental content has been rekindled; and there is a "'new look'" in the investigation of mental content. The "'new look'" consists in "'thought sampling,'" which Professor McClelland explains by using a blood count analogy.

It is Professor McClelland's belief that a psychology of mental content is possible and that we are moving in the direction of its development. To work fruitfully within such

a framework the psychologist will need a broad background in general education. We will need to know something about economics, political science, history, art, and the like. In short, "we simply cannot afford to be naive and pretend that research scholars in these fields have nothing to tell us."

Professor McClelland has presented his case in a clear and straightforward way. On several points we are in definite agreement with him. Psychologists should be interested in what organisms perceive, what they learn, etc. The what of behavior is an integral part of the psychological event. Furthermore, psychologists do have much to learn from such disciplines as economics, political science, history, etc. For too long we have neglected these areas of knowledge and research and, consequently, have failed to profit from developments in them.

Where we differ with Professor McClelland is in his identification of the what of behavior as mental content and in his implied suggestion that this aspect of the psychological event is located in the individual's head. This is mentalism, pure and simple. The what of behavior, objectively describable in stimulus-response terms is reduced to a phantom, a spook, a bit of mental stuff floating about inside the individual's head.

Consider, for example, the case of the individual looking at a yellow flower growing in a public park. We ask him: "What do you perceive?" He replies, "I perceive a yellow flower." Objectively, we may describe the event as a perceptual interaction. A particular organism is interacting with a particular stimulus object in particular circumstances. The event is one of identifying or discriminating the properties of this object. The what of the event is the yellow flower, the object interacted with. It is growing in the earth of a public park. It is not mental content, nor is it located in the individual's head. Decapitate the individual and the yellow flower continues to thrive. True enough, one person may identify the object as simply a "yellow flower" and another may discriminate it as a "lovely yellow flower." But this difference in behavior points only to the fact that

perceptual events are conditioned by such factors as (a) the past behavior history of the organism, (b) the organism's hygienic condition, (c) setting features, etc.

In a similar manner, an objective stimulus-response psychology can adequately account for the what of any given psychological event. Thus, when our flower fancier thinks on the following day of the perceptual event described above, the what of his behavior is the total event of the preceding day, acting upon him through substitute stimulation.

One last word remains to be said. This concerns the contents of an individual's head. Many psychologists have claimed this part of the body as the locus of such mental elements as ideas, thoughts, wishes, etc. Internal biological examination of the head yields no evidence for this claim. One is led to suspect, therefore, that it is based not on an act of discovery but on an act of human creation.

Let us leave the workings of the human head to the researches of the trained biologist. Perhaps then, we will be able to follow the advice of the German poet, and "leave the heavens to the angels and the sparrows."

Paul Swartz

Comment by Professor McClelland

The following note was received from Professor McClelland in response to our invitation to comment on the critique of his article:

It is Dr. Swartz who is bothered by "spooks," not me. I simply couldn't care less "where" thoughts are located. The thoughts I am talking about, whether they be of flowers or philosophy, are, operationally speaking, written on a piece of paper and I do content analysis of them "there." If a philosopher or Dr. Swartz wants to quarrel with this statement, let him make the most of it. It will not change what I do one whit (or, I suspect, what any other operating psychologist will do). The only thing that surprises me is

that Dr. Swartz apparently took seriously what I thought was obviously a metaphor when I used the phrase "what goes on in people's heads." I'm sorry I misled him by a figure of speech into reviving talk of ancient issues which I thought had been long dead for operating psychologists, though perhaps not for philosophers. I guess I would defend it as a figure of speech on the grounds that thoughts seem better located in the central nervous system than in the big toe or the heart, but is it worth arguing about a figure of speech?